MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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PROFESSOR CAYLEY, President, in the Chair.

E. A. Sturman, Esq., Annerley;
John Garbutt, Esq., Leeds;
William Tomlinson, Jun., Esq., Nelson, New Zealand; and
T. W. Backhouse, Esq., West Hendon House, Sunderland,

were balloted for and duly elected Fellows of the Society.

Transmission of Free Messages on Astronomical Subjects over the Transatlantic Cables.

A very important concession has been made to the Smithsonian Institution by the Directors of the Associated Trans-Atlantic Cable Companies, who have agreed to transmit gratuitously between Europe and the United States, a limited number of short messages on astronomical subjects. Under this arrangement two telegrams have already been received from the United States by the Astronomer Royal, who on his part has undertaken, at the request of Dr. Henry, Secretary of the Smithsonian Institution, to forward from Europe any message announcing an important astronomical discovery. The Directors of the Associated Companies have consented that ten messages, of ten words each, may be sent free over the cables annually. This liberal concession on the part of the Directors cannot be too highly appreciated by astronomers generally, and especially by the Fellows of this Society.

In conformity with this agreement the Astronomer Royal will

be prepared to forward any important astronomical message, limited to *ten* words, which may be sent to him for this purpose from the principal European astronomers.

Royal Observatory, Greenwich,
April 8, 1873.

Chinese Observations of Solar Spots. By Mr. John Williams.

Having lately become possessed of a copy of the celebrated Encyclopædia of Ma Twan Lin, I found, on examining the astronomical sections of that work, a considerable number of observations of solar spots, extending over a period of 904 years, and, considering that some account of these ancient observations might be of interest to the Society, I have requested permission to lay before you a translation of them, in the hope that they may be of some service to such of the Fellows as may be more particularly engaged in the investigation of these peculiar phenomena.

The Encyclopædia of Ma Twan Lin is considered not only by the Chinese themselves, but also by such European Sinologists as have had occasion to refer to it, as a most remarkable and trustworthy work. Many eminent writers speak of it in the highest terms of approbation, and among these I take the opportunity of calling your attention to the opinion of Abel Remusat, the author of a well-known and excellent Chinese grammar, who, after giving, in that work, an account of a number of the best Chinese works on different subjects, concludes thus:—"And above all, the excellent work of Ma Twan Lin, entitled *Wan Heen Tung Kao*, the finest monument of Chinese literature, a vast collection of memoirs on all sorts of subjects, a treasure of erudition and criticism, in which all the materials that Chinese antiquity has left us, relating to their religion, legislation, rural and political economy, commerce, agriculture, government, natural history, physical geography, and ethnology, will be found, brought together, classed and discussed, in admirable order, method, and lucidity,—in short, a production which, as I have had occasion before to remark elsewhere, is in itself a complete library, and which, did their literature offer nothing else of value, would deserve that the Chinese language should be acquired, were it for no other purpose than to read this work."

My copy of this highly esteemed work is in 100 Chinese volumes. The matters treated of are arranged under 24 principal heads. These are divided into 348 sections or chapters, which have also numerous subdivisions. The astronomical portion consists of 17 chapters, Nos. 278 to 295 inclusive. The subjects treated of are—The general distribution of the stars in the heavens, comprising the asterisms in the three great spaces and the twenty-eight stellar divisions, the whole of which are

enumerated and fully described; thus forming a complete Chinese catalogue of stars.

The degrees and mansions of the twelve zodiacal divisions follow, and it may be here observed that the ancient Chinese zodiacal names alone are given. The course of the Milky Way comes next, followed by a general account of the Sun, Moon, and five planets. Next we have extraordinary appearances in the stars and in the heavens generally; these, however, consist principally of accounts of halos and vapours or extraordinary configurations of the clouds.

These are followed by a long account of eclipses of the Sun, commencing with that recorded in the Shoo King, said to have occurred B.C. 2158, of which I gave an account in the *Monthly Notices*, vol. xxiii., and ending with one in A.D. 1223, the number being more than 600; some account of these may, if agreeable to the Society, form the subject of a future communication. We have next extraordinary appearances connected with the Sun, among which are the observations of solar spots, which form the subject of the present paper. These are followed by eclipses of the Moon and singular appearances connected with that body. Next are observations of comets. These have been translated in the work I lately published on Chinese Comets. Prognostications or astrological deductions from the motions and positions of the heavenly bodies come next, followed by observations of shooting or other moving stars; they extend from the Chow dynasty, which commenced B.C. 1122, to about A.D. 1230. After which we have accounts of stars seen in the day-time. These are principally *Venus* and *Jupiter*. Next, an account of auspicious stars, then of temporary stars, and lastly, of extraordinary halos and rainbows.

The observations of solar spots which form the subject of this paper extend from A.D. 301 to 1205, a period of 904 years. They, however, amount to but 45, a number which, considering the time over which they are spread, must appear very small. But it must be recollected that naked-eye observations of the solar spots must of necessity be of very rare occurrence, and, as there is no mention of the employment of coloured or smoked glasses to take off the glare of the Sun, they could only be made under the favourable circumstances of a fog, and the Sun rarely presents us with spots sufficiently large to be seen, even then, without optical assistance. Sir John Herschel, in his *Outlines of Astronomy*, remarks that, at least, on two occasions before the invention of telescopes, spots had been seen with the naked eye, and it appears evident to me that these were considered by him as occurrences of a very unusual kind. Here, however, in Ma Twan Lin's work, we have a record of 45 observations of these objects, some of which were seen for several days following. Many of these spots are recorded, as resembling plums of different kinds and sizes, and others are said to have been as large as a hen's egg, and one is mentioned as having

resembled a duck. This may remind us of one of the observations of the late Capt. Shea, who describes a group of spots as being like a ship, and also of the drawing of a group of spots exhibited by Mr. Howlett some years ago, which bore a grotesque resemblance to a human skeleton. Most of the instances of their having been seen for several days in succession are within the bounds of possibility. One, however, which is said to have been seen for 19 days, could never have been observed for that length of time on account of the now well-known period of the rotation of the Sun, and it is singular that no conclusions were attempted to be drawn from the peculiar phenomena thus presented to their eyes, nothing of the kind being to be found in their writings.

These observations are continued in the supplement to Ma Twan Lin's Encyclopædia, published since the accession of the present dynasty in 1644, in which the subjects treated of in the former work are brought down to the date just mentioned. I have had no opportunity, lately, of consulting this work, but I have found in the history of the Ming dynasty, that which preceded the persent one, many observations of the solar spots, the latest being dated Nov. 29, 1638, only six years before the accession of the reigning dynasty, the Tsing. On the present occasion I shall confine myself solely to the observations contained in the original work of Ma Twan Lin. They are possibly of but little value in themselves beyond the mere fact that they were made by the Chinese astronomers at the dates specified, but I must here observe that, as it is stated in our astronomical works, that among Europeans, Galileo in 1610 was the first observer of the solar spots, it follows that the Chinese astronomers were far earlier in their observations of those remarkable objects. In fact, these very observations now placed before you were printed and published in the first edition of Ma Twan Lin's Encyclopædia, which edition appeared in 1322. I must also express my conviction, founded, after a careful and critical examination, on the peculiar appearance of the characters, the date 1322, the paper and other indications, that in the volumes in my possession, I have really a copy of the first edition of Ma Twan Lin's work,* in which opinion I

* As, when this paper was read, some doubt was expressed as to the correctness of this statement, I take this opportunity of giving the reasons why I came to that conclusion. First, as respects the date 1322. Chinese books generally have the date at the end of the preface. When a second edition is published another preface is added to the preceding one with the date of the issuing of that edition. In the present instance there is a preface with a date which appears to be that of the *completion* of the work. This is followed by a summary of the arrangement of the subjects, at the end of which is another date evidently that of the *first publication*. That of the preface is said to be the first year of the Epoch 大至 Che Ta. That of the summary to be the second year of 冶至 Che Che. On reference to the chronological tables appended to my work on Chinese comets, the epoch Che Ta will be found to be that of the Emperor Woo Tsung of the Yuen dynasty, commencing 1308, which is therefore the date of the completion of the work. Che Che is the epoch of a succeeding Emperor, Ying Tsung, which epoch commenced 1321, the second year of which is consequently 1322, the date of the

am supported by the Rev. J. Summers, Professor of Chinese, King's College, London. Hence we find that these observations were published 288 years before Galileo's observations in 1610. I may also remark that, as these Chinese observations commence in A.D. 301, their astronomers had detected the spots in the Sun 1308 years before the assumed first discovery by Galileo. It must, however, be observed, that, previous to the commencement of the seventeenth century the intercourse between China and Europe was extremely limited, and little or nothing was known by Europeans either of the language, or of the general literature of that country until towards the close of that century. It was, therefore, next to impossible that Galileo could have known anything of what the Chinese had done so long before him, and he is therefore fully entitled to the credit given him by his contemporaries as regards the solar spots. These circumstances render these early observations of the Chinese historically curious; and if they have no other merit, they tend to establish the fact, with some appearance of certainty, that the physical constitution of the Sun has undergone little or no alteration since A.D. 301 to the present time, a period of 1572 years.

In extracting these observations I have confined myself merely to the year, month, and day of their occurrence. In the original we have the name of the Emperor and other particulars which are unimportant; all that is required being the correct date, which, in every instance, has been carefully reduced from the Chinese mode of expressing it, to our reckoning, by means of the tables I published in my work on Chinese cometary observations. To this I have added, where necessary, the incidental remarks relating to the appearance or size of any particular spot, and the number of days, on which it was visible. I have, however, left out the astrological deductions, as in several instances the death of an Emperor, or some other important personage, is said to have shortly followed the appearance of a spot. In short, I have rendered them as concisely as possible, and have only to express my hope that they may not be found entirely useless in the investigation of these remarkable phenomena.

publication. There is no other date to be found in the work. The characters, again, are peculiar, and appear to bear the same relation to those of more recent date, that the typography of the reigns of James I. or Charles I. does to that of the present time. The paper, also, is thicker and coarser than that since employed, and is not only in many instances extremely tender, but is also wholly wanting in that peculiar softness and silky feel which generally distinguish the paper of more recent times. Surprise was also expressed that books of that age should be in such good condition; but, as is well known, *our* early printed books are often to be met with in perfect preservation. I myself have books printed at the close of the fifteenth and beginning of the sixteenth centuries that are in excellent order, so that there is no great wonder that these Chinese volumes should also be in good preservation, particularly as they have evidently been recently very carefully mended, and put into good condition by having been rebound.

Chinese Observations of Solar Spots from A.D. 301 *to* 1205, *extracted from the Encyclopædia of Ma Twan Lin*, vol. lxxxiii. chapter cclxxxiv.:—

A.D.		
301	October 20	
321	March 8	
322	March 11	
342	September 3	
354	November 7	Large, like a hen's egg.
355	April 4	Like a peach.
359	September 8	Like a hen's egg.
369	November 3	
370	March 29	Like a large plum.
372	November 30.	
373	November 26	Like a small plum.
374	April 6	Like a duck.
374	November 27	Like a hen's egg.
388	April 3	Like a small plum.
389	July 17	Like a small plum.
396	December 8	
400	December 24	
577	December 30	Like a cup.
826	March 25	
832	April 14	
837	December 15	Like a hen's egg.
841	December 31	
874	No month or day.	
974	February 2	
1077	March 6	Like a large plum.
1078	March 10	Like a plum. The text says seen for 19 days.?
1079	January 11	Like a plum. Seen for 12 days.
1079	March 28	Like a plum. Seen for 10 days.
1104	November 12	Resembling a date.
1112	May 3	As large as a chestnut.
1118	November 17	Like a large plum.
1120	July 31	Like a date.
1123	January 5	Like a large plum.
1129	April 21	
1129	December 16	
1131	March 12	Like a large plum. Seen for 3 days.
1136	November 24	Like a large plum. Seen for 10 days.
1137	February 27	Like a large plum. Seen for 10 days.
1138	March 17	Resembling a large date.
1138	November 26	
1186	July 22	Like a date.

A.D.			
1193	December		
1200	August 23	Like a date.	Seen for 6 days.
1201	January 4	Like a date.	Seen for 12 days.
1205	February 7	Like a date.	Seen for 13 days.

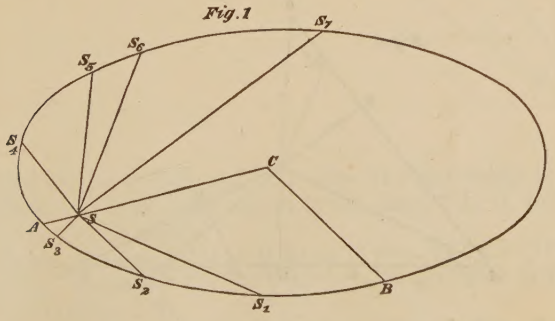
A Geometrical Investigation of the Orbit of a Double Star.

By J. M. Wilson, Esq.

Having devoted spare hours occasionally to working out the orbits of double stars, I was led to endeavour to find a purely geometrical solution of the problem, "Given the orbit as projected on the plane of the sky, and the position in it of the star of reference, to find the inclination and position of the real orbit." It must be remembered, of course, that the actual orbit may be in any plane, and that the star of reference is assumed to be in its focus in that plane, but that when seen obliquely, the eccentricity of the orbit is in general changed, and the star always displaced from the focus of the projected ellipse. The problem, then, is to determine, from the amount of displacement of the focus, the position of the real ellipse. Herschel has given in the Astronomical Society's *Memoirs*, vol. v., analytical formulæ for this problem; that is, formulæ for computing the longitude of the node, the inclination of the orbit, and the angle of position of the perihelion; but, as far as I am aware, no elementary geometrical solution has been given.

Let S_1, S_2 ... S_n, be positions of one star with reference to another star S at different dates, the angle being got directly from observation, and corrected by the interpolating curve, and the distance being computed therefrom by Herschel's method; and let the ellipse in fig. 1 be found to pass through, or as nearly

Fig. 1



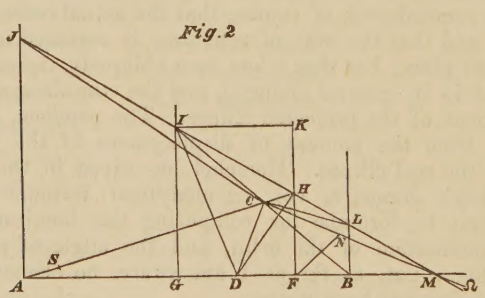
as may be through them all. Find C the centre of this, the apparent ellipse. Join C S, and produce it to meet the ellipse in

A. This must be the projection of the axis major of the real orbit. Draw the projection of the *latus rectum*, which will be the chord that is bisected in S, and draw C B parallel to it; C B will be the projection of the minor axis of the real orbit. Also $\frac{C S}{C A}$ is unaltered by projection, and is therefore the eccentricity of the real ellipse; and therefore the ratio of the axes of the real ellipse is known. Draw C B′ at right angles to C A, and take S B′ = C A, then C A, C B′ are proportional to the semiaxes of the real ellipse.

Then the problem is evidently simply this, to draw from C two straight lines, such that their projections shall be C A and C B, such that they shall include a right angle, and such that their ratio shall be that of C A to C B′. These lines will be the semiaxes of the real orbit, and will determine its plane.

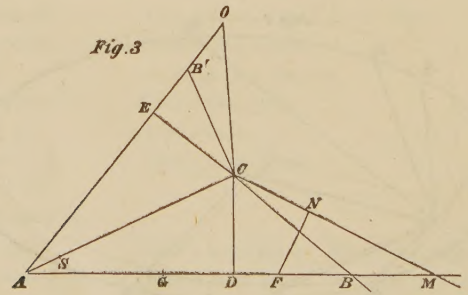
Make (in fig. 2) a perspective view of the triangle A C B.

Fig. 2



Draw A J, B L lines through A, B, perpendicular to the base; then the extremities of the semiaxes are to be on those lines. Draw C D perpendicular to A B (in figs. 2 and 3), and C E perpendicular to A B′.

Fig. 3



Divide A B in F, so that A F : F B :: A E : E B′, by joining B B′ and drawing E F parallel to B B′.

Divide A F in G, so that B F : F G :: B′ E : E C.

Through F, G, (in fig. 2) draw lines perpendicular to the base.

Determine two lines, F H, H K, such that their rectangle shall equal G F . D F, and the difference of their squares, $F H^2 - H K^2$, shall equal $G F^2 - D F^2 - C D^2$, and set off these lines F H, H K, along the line through F perpendicular to the plane A B C. Draw K I parallel to G F to meet G I in I; join H I, and produce it to meet A J, B L in J, L; and join C J, C L. Then C J, C L are the semiaxes of the real orbit.

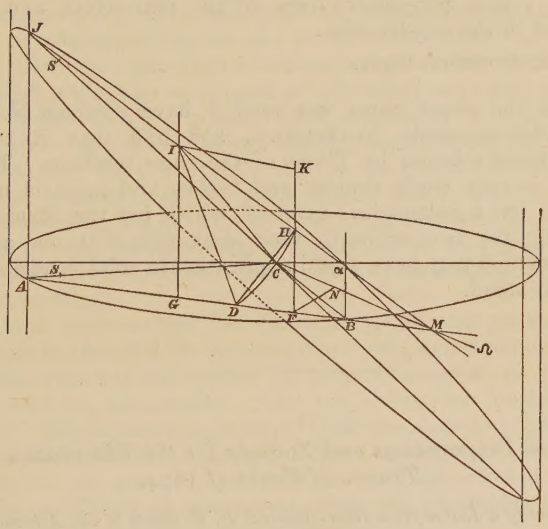
It must be shown that C J, C L, are in the required ratio C A : C B′, and that they include a right angle.

Join C I, C H, I D, D H.

Because F H, H K = D F, I K, by construction; therefore D F : F H :: H K : I K; and therefore the triangles D F H, H K I, are similar; and the angle D H F is complementary to K H I, and the angle D H I is a right angle.

And since C D is perpendicular to the plane I D H, I H is

Fig. 4.



at right angles to C D, and it is also at right angles to D H, and therefore I H is at right angles to the plane C H D, and therefore I H C is a right angle.

Again, because $F H^2 - H K^2 = G F^2 - D F^2 - C D^2$, by construction, therefore $C D^2 + F H^2 + D F^2 = G F^2 + H K^2$, that is $C D^2 + D H^2 = I H^2$.

But since C D H is a right angle, $C D^2 + D H^2 = C H^2$, therefore I H = H C.

And because L H : H I :: B F : F G :: B′ E : E C, therefore

L H : H C :: B′ E : E C, and therefore the triangle L H C is similar to the triangle B′ E C.

And because L H : H J :: B F : F A :: B′E : E A and L H : H C :: B′ E : E C, therefore H J : H C :: E A : E C, and therefore the triangle C H J is similar to the triangle C E A, and the whole triangle L C J is similar to the triangle B′ C A, that is, C J : C L :: C A : C B, and L C J is a right angle. The position therefore of the orbit is found.

To find the Line of Nodes.—Produce J L to meet A B in M. Join C M. C M is the line of nodes, and the angle it makes with C A or C B is therefore determined (fig. 1).

To find the Inclination.—Draw F N perpendicular to the line of nodes. Then if γ is the inclination, $\cot \gamma = \frac{N F}{F H}$ and γ is therefore known.

To find the Position of the Perihelion on the Orbit.—This is the angle M C J = 90° + M C L.

Draw M O parallel to B B′ to meet A E in O; join C O. Then the angle M C L is equal to the angle O C B′.

Fig. 3 is a perspective view of the two orbits, and of the lines used in the construction.

Temple Observatory, Rugby.

Since the above paper was read I have received No. 1227 of the *Astronomische Nachrichten*, and find that it contains a geometrical solution by Thiele of the same problem. Thiele's solution is very much simpler and better, and mine is nothing therefore but a geometrical exercise, which has the advantage of exhibiting the two orbits in their connexion with one another, and its method may have useful application in solid geometry, and so I let it stand.

American Preparations and Stations for the Observation of the Transit of Venus of 1874.

(*Extract of a Letter from Rear-Admiral B. F. Sands to the Astronomer Royal.*)

In answer to your letter of February 27, I have much pleasure in making known to you our plans for the observation of the Transit of *Venus*, so far as they have been matured.

As to the mode of observing, our main reliance will be upon photographs of the Sun, with *Venus* on his disk, taken on the plan described by Professor Newcomb in Part I. of the "Papers relating to the Transit of *Venus*." In applying this plan, it will be admissible to choose stations where the Sun shall be 10° or more above the horizon during the entire period of the transit, and

where the effect of parallax shall be to change the average distance of centres as much as possible during the transit. The favourable Northern stations will all be selected on the coast of China, Japan, and Siberia; one probably at Wladiwostok (Lat. 43°7′; Long. 8^h48^m); one at or near Yokohama; one near Pekin, or between Pekin and the coast; and the fourth somewhere in Japan, China, or the adjacent islands.

In the Southern hemisphere satisfactory stations are much more difficult to find. Our choice seems to be confined to Kerguelen Land, Tasmania, Southern New Zealand, and Auckland or Chatham Island, subject to the consent of the British Government. The most favourable of these stations is probably Kerguelen Land, which you mention among those you purpose to occupy yourself, and which I believe the Germans also intend to occupy.

It is a delicate question whether there are not very grave objections to having so many stations together, the answer to which must mainly depend on whether similar methods of observations are to be employed by the different parties. The force of the objection is greatly diminished by the circumstance that our method of photographing is not to be employed by any other nation. Still the comparative inaccessibility of that point allows me to speak with little confidence of our ability to occupy it.

In addition to these photographic stations, it is our wish to comply with your desire that we should occupy a contact station in the Pacific. Here we prefer one of the Sandwich Islands, as distant as possible from the point which you may select. The objection to occupyng a station so near yours seems to be counterbalanced by the very favourable conditions of that group, both astronomically and meteorologically, and by its accessibility from our western coast.

As both contacts will be visible from all the photographic stations, it is intended to observe them with 5-inch equatoreals, with clock-work and micrometer for measuring cusps, one of which will be sent to each station. As the factor for "ingress accelerated" will be about as great at Wladiwostok and at Yokohama as it will be at Tahiti, it does not seem necessary to occupy the latter station in addition, and besides, only one contact can be seen either at Tahiti or Marquesas, while the Asiatic stations are about equally favourable for both contacts.

Each station will also be furnished with a portable transit, accompanied by clock and chronograph, for the determination of local time. This transit will be supplied with a fine spirit-level and declination micrometer for use as a "zenith telescope." For longitude, we shall probably depend mainly on occultations of small stars to be observed with the 5-inch telescopes. It is hoped by careful watching to observe eight or ten occultations per month, mostly when the Moon is near her conjunction, and while she is passing the Milky Way. It is believed that occultations are much more free from systematic errors than Moon-culminations.

The numerous old determinations of the Transatlantic longitudes by the latter method, most of which may be found in Gould's paper, do not encourage us to rely upon it.

U.S. Naval Observatory, Washington,
March 19, 1873.

Passage de Vénus: Méthode pour obtenir photographiquement l'instant des contacts avec les circonstances physiques qu'ils présentent. Par M. Janssen. (Abstract.)

This paper, which appears in the *Comptes Rendus* for March 17, 1873, explains an apparatus contrived by M. Janssen, for recording photographically the exact moment of the contacts of *Venus* with the Sun's limb, with reference to the approaching transit of 1874. It is intended that the contrivance should produce, at the moment when the contact was expected, a series of photographic records at short and regular intervals; so that the photographic image of the contact would necessarily be comprised in the series, and would at the same time give the exact instant of the phenomenon. The details of the contrivance are as follow:—

"La plaque sensible prend la forme d'un disque; elle se fixe sur un plateau denté qui peut tourner autour d'un axe parallèle à l'axe de la lunette ou du télescope qui donne l'image du soleil. Le disque est excentré de manière que les images se forment vers la circonférence. Devant ce disque, un deuxième disque fixe formant écran, est percé d'une petite fenêtre pratiquée de manière à limiter l'impression photographique à la portion de l'image solaire, où le contact doit se produire.

"Le plateau circulaire qui porte la plaque sensible est denté, et mis en rapport avec un petit appareil d'échappement commandé par un courant. A chaque seconde, le pendule d'une horloge interrompt le courant, le plateau tourne de la valeur angulaire d'une dent, ce qui amène sous la fenêtre une portion non impressionnée de la plaque, où une nouvelle image du bord solaire vient se peindre. Si le disque porte, par exemple, 180 dents, la plaque pourra reçevoir 180 images du bord solaire. On pourra donc commencer les photographies une minute et demie avant l'instant présumé du contact (instant que le spectroscope peut d'ailleurs indiquer pour le premier contact extérieur). Quand la série relative à un premier contact est obtenue, la plaque sensible est retirée et remplacée par une autre qui donnera le deuxième contact, et ainsi pour les quatre. Ces plaques sont ensuite examinées à loisir avec un microscope; l'instant du contact est donné par l'ordre de la photographie qui, dans la série, en présentera l'image.

"On comprend qu'il est nécessaire de régler le temps de pose. On y parvient au moyen d'une languette métallique munie d'une

fente variable qui forme écran devant la fenêtre du disque observateur, et qui, par une disposition mécanique particulière, découvre la fenêtre pendant la fraction de seconde reconnue convenable dans les essais préliminaires."

M. Janssen remarks that this note is intended only to indicate the principle of the method, and that he hopes to give further details and diagrams in a future communication.

Note on the Approaching Transit of Venus, with special reference to the probability of absolute failure through the want of a due number of Southern Stations. By Richard A. Proctor, B.A. Cambridge.

I observe that, in Mr. Penrose's valuable contribution to the discussion of this subject, he overlooks certain considerations relative to Southern stations, which are I think extremely important. In fact, none of the papers on the transit which I have hitherto seen dwell with adequate force on the circumstance to which I am about to invite attention.

It does not seem to be noticed that there is great risk of the whole series of observations of the transit being rendered useless for want of an adequate number of Southern stations.

At Kerguelen's Island bad weather is more than likely; it is all but certain, as any one can ascertain by studying Sir J. C. Ross's narrative of his Southern voyages. The same remark applies to Crozet Island, which, however, has not as yet been selected by any country. Observations in Tasmania and South Australia will not be of great value. There remain then only Rodriguez, Mauritius, and Bourbon, for retarded ingress, and the New Zealand stations for accelerated egress. If bad weather prevails over the groups of island first named, and also in New Zealand, it is all but certain that the whole affair will end in complete failure. Be it noticed also that good weather in either region would only avail for Delisle's method, and would be insufficient if bad weather prevailed at but one region of Northern stations.

Let the chances, as matters are at present arranged, be but carefully weighed, and I feel assured it will be recognised that there is very great occasion for anxiety as to the result. Northern chances outweigh Southern chances ten to one, but the balance counts for nothing. Success depends altogether on observations being made in both hemispheres.

If however we consider only the chances relating to three out of the four methods on which astronomers as a body place reliance, the matter assumes a much more serious aspect. The four methods are Delisle's, Halley's, the photographic method, and the direct method. Take now the last three. For the application of

these methods the Russians have made ample provision, so have the American astronomers, and the Germans will occupy at least one station, Tchefoo, specially for these methods. Every preparation is being made, in fact, for Northern work (except only that our North Indian region, available for these methods as well as Delisle's, is not sufficiently provided for). But now what is there to balance all this, in the Southern hemisphere? *Of really first-class stations there are but three which have even been mentioned,—viz., Crozet Island, Macdonald Island, and Kerguelen Island. Of these only Kerguelen Island has been actually selected; and here bad weather is almost a certainty. Of the other stations Canterbury (N.Z.), Chatham Island, Bourbon, Mauritius, and Rodriguez, it is only necessary to remark that they are very inferior for these three important methods.*

It is on this account chiefly that I have been earnest in my appeal for the occupation of Antarctic and sub-Antarctic stations. If anything were required to add to my anxiety on this subject, it would be found in the manifest reliance placed by Russia, America, and Germany, on the methods in question.

I am concerned to think that reconnaissances over the regions between Kerguelen Island, Enderby Land, Possession Island, and Auckland Island, may be absolutely necessary for a proper choice of stations; that such reconnaissances might have been made since I first dwelt on these matters four years ago; and that possibly had I been earnest in advocating these considerations during the last four years, either Great Britain or America might before this have found suitable observing stations in the above named region. I judged it best simply to indicate the state of the case and wait. I fear I may have been mistaken, though it is difficult to see what could have been done until the approach of the event itself and the declared intentions of other countries enforced attention to the circumstances I have touched upon. I trust it may still not be too late to provide for an adequate number of Southern stations sufficiently far apart to give proper chances of success. I do not hesitate to say that in my opinion the provision hitherto made is altogether inadequate, so far as Southern stations are concerned.

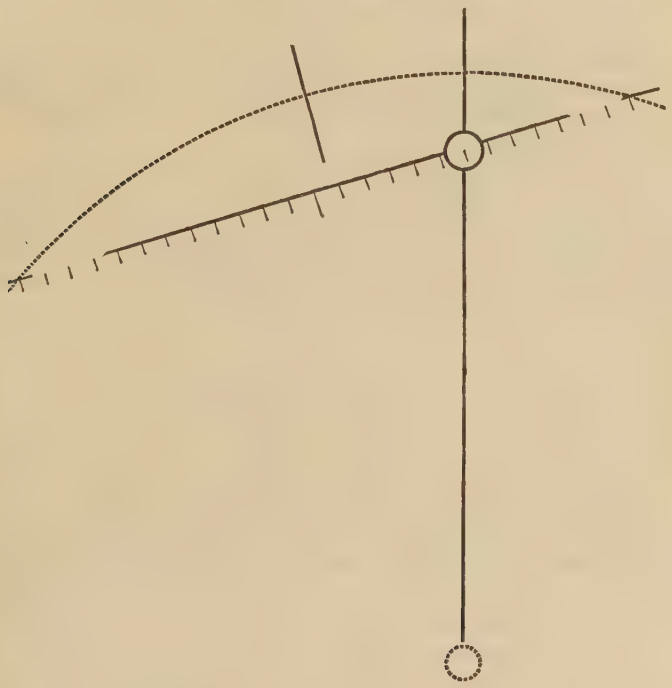
A Graphical Representation of the Circumstances of the Transit of Venus in 1874, *with a view to show the sufficiency of this method for the purposes of Prediction.* By F. C. Penrose, Esq.

Notwithstanding the admirable diagrams which Mr. Proctor has laid before the Society, it may still be interesting that the subject should be looked at in various ways. I can hardly suppose but that Mr. Proctor, considering the great attention he

has paid to the matter, may have anticipated me in this, but the subject is so important that some repetition may be excused.

The method I have used is the same, to all intents and purposes, as that which I have used in predicting occultations and eclipses. Indeed, until dealing with the refinements which will be necessary in discussing observations when we get them, it is rather a simpler case than an eclipse or a planetary occultation.

The woodcut is a perspective projection of the phenomenon. The plane of the projection is at right angles to the Earth's radius vector at the moment of conjunction in R. A. of ☉ and ♀. The distance is the radius vector of ♀, and the point of



sight (as it is called), and which here is the same as the projecting point, is the centre of the Sun. The dotted line is the Sun's limb, the inclined line is the path of ♀. The vertical line is the line of conjunction in R.A. The circle there represented is the true size of *Venus* in scale with the Sun; the dotted circle below is the Earth as projected on to the plane of the picture, and therefore obviously measuring $P - p$, where P and p are the horizontal parallaxes of *Venus* and Sun respectively.

This diagram suffices for all the purposes of prediction; in

proof of this, I submit certain results obtained from it with the calculations given in the *Nautical Almanac*.

Internal contact considered in every case. The times mentioned are local mean time.

	Graphic Method. h m	Nautical Almanac. h m
Alexandria: Egress.. ..	20 7·0	20 7·0
Kerguelen: Ingress.. ..	19 3·4	19 3·0
Egress	22 28·7	22 28·7
Auckland, New Zealand: Egress	5 27·7	5 27·5
Honolulu: Ingress	3 33·3	3 33·0

Least Distance of Centres.

$16^h\ 6^m\ 35^s$ G.M.T. against $16^h\ 6^m\ 32^s$.

I have no doubt—indeed I am sure—that the graphic process is easily susceptible of still greater accuracy, and this, I think, confirms the high expectations formed by Dr. De La Rue and others from photography.

The method is simply this: for any chosen place apply first roughly a radius measuring $S - s$, and see to a few minutes on the planet's time path (which can be done at once) where it cuts that line, and what is about the time before or after conjunction. For instance, let us take Honolulu, Latitude 21° 18′ 24″, Longitude, $10^h\ 31^s\ 30^m$. Then form the following equation:—

	h m s	h m s	h m s	
G.M.T.	2 50 8	Before ☌ 14 9 0	17 3 0	Sun's R.A. corresponding.
	Acceleration	0 2 20	10 31 30	Longitude *West*.*
S.T. at G. M. Noon		17 8 18	3 45 8	Hour Angle *East*.
		31 19 38	31 19 38	

The terrestrial parallax is now very easily found, either with the help of such a diagram as I have given in my work on Occultations, &c., or quite as readily by the solution by the aid of the slide rule of—

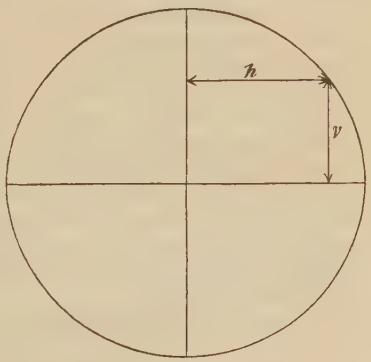
$$v = \sin\lambda \cos\delta + \cos\lambda \sin\delta \cos \text{hour-angle}.$$

$$h = \cos\lambda \sin \text{hour-angle}.$$

Such Southern stations are best which, like Kerguelen, see the ingress in the morning, and the egress towards noon; and for Northern Stations exactly the reverse holds good; as, for instance, Nertschinsk. So much so is this the case for the Southern stations that Kerguelen—latitude about 49°—has a course only one minute longer than Possession Island, so much to the south of it; whilst Enderby, of nearly the same latitude as Possession Island,

* If longitude is west it goes into the second column. If east into the first column. If the hour-angle which is required to balance the equation is found in the right-hand column it is east, otherwise west.

would (supposing that a station there were practicable) give a course 4 minutes shorter than Kerguelen. The cause of this is easily seen. Suppose two places on the same parallel of latitude, but of such difference of longitude that one should see the ingress at noon and the egress in the afternoon, whilst the other sees the ingress in the morning and the egress at noon. It is obvious by looking at the diagram that the path of the first is very oblique to the direction of the radius S—*s*, whilst the other is much more nearly in the same direction, it consequently produces much more effect in practically reducing this radius, and cutting off a shorter segment from the time-path. This is in favour of a Southern station. At a Northern station, on the contrary, where a lengthened path is desired, such positions as produce the least possible reduction of the radius S—*s*, and consequently the reverse condition of longitude, are most desirable. This consideration explains the great value of Nertschinsk, in addition to its high Northern latitude; but there are places in Japan, and especially on the coast of the mainland near Lake Kinka, which are nearly as good, and might very likely be better stations for observation. A similarly constructed diagram for the transit of 1882 (the elements of which have been kindly communicated to me by Mr. Hind) has enabled me to confirm all that Mr. Proctor has said about the great superiority of the transit of 1874 to that of 1882. The utmost result which could be got for Delisle's method in that year would be under 17 minutes. In 1874 the combination of Kerguelen and Honolulu gives 23 minutes; and if Crozet's Island could also be occupied, it would be very greatly to be desired, because, although not quite so southerly as Kerguelen, the consideration of the path which I have mentioned acts more in its favour, and the time-path would be about six-tenths of a minute more favourable, either for Delisle's or Halley's process. The altitude at ingress is perhaps not quite high enough, but still 13°. Everything, at any rate, seems in favour of straining a point to utilise the transit of 1874 as much as possible. Indeed I am inclined to expect that the best results in 1882 will be from photography, especially with the help of the experience of the coming year. I am not competent, nor do I venture, to offer any opinion whether Halley's or Delisle's method is the best; but when so little is asked as to do our best to make sure both of Kerguelen and Crozet's Island, and to do something to support Nertschinsk in or near Japan, for the sake of



having two strings to our bow; it is at any rate worth urgently raising the question, and speaking before it is too late. On one point my graphical investigation has led me to a consideration which at first sight, at any rate, seems to be in favour of Delisle's method as against Halley's, and it is this: if there is any error in the declination of *Venus*, or the measure of the semidiameters, it would entail a greater error on Halley's than on Delisle's method. As far as I have thus far made out, an error of 1^s in these measures would affect the time-path to the extent of about 9^s for Halley's method, and about 5^s for Delisle's; but this is perhaps only an additional reason why the two methods should be resorted to, if only for the sake of clearing up any such possible error, or errors.

April 9, 1873.

Graphical Method for Determining the Motion of a Body in an Elliptic Orbit under Gravity. By Richard A. Proctor, B.A., Cambridge.

The student of astronomy often has occasion to determine approximately the motion of bodies, as double stars, comets, meteor systems, and so on,—in orbits of considerable eccentricity; and therefore a graphical method for solving such problems in a simple yet accurate manner will probably be of use to many readers of the *Notices*. The process now to be described is, so far as I am aware, a new one, (though I have an indistinct recollection of a paper, by Mr. Waterston, if I remember rightly, suggesting that part of the construction which relates to the curve of sines).* Of course it involves no new principles. By its means a figure, such as the lithographed diagram illustrating this paper, having once for all been carefully inked in on good drawing card, the motion of a body in an orbit of any eccentricity can be determined by a pencilled construction of great simplicity, which can be completed (including the construction of the ellipse) in a second or two.

Let A P A′ be an elliptical orbit of which A C A′ is the major axis, S being the centre of force, so that A is the aphelion, and A′ the perihelion. Let a be the half major axis; e the eccentricity C S; H half the periodic time, and T the time in which the body moves from A to P.

On A A′ describe the auxiliary semicircle A b A′.

Then

$$\begin{aligned} T &\propto \text{area A S P} \propto \text{area A S Q} \\ &\propto \text{area (A C Q + S C Q)} \\ &\propto a\,(\text{A Q}) + e\,(\text{Q M}) \\ &\propto \text{A Q} + \frac{e}{a}.\,\text{Q M}. \qquad (\alpha) \end{aligned}$$

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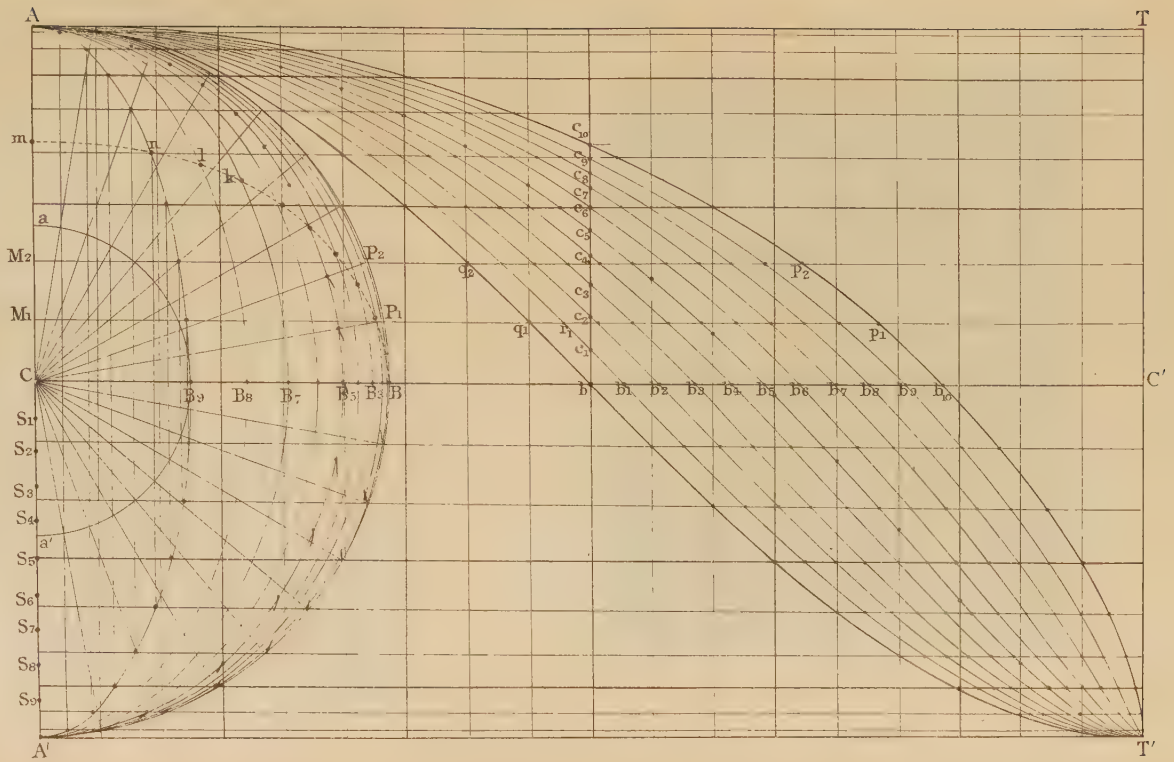
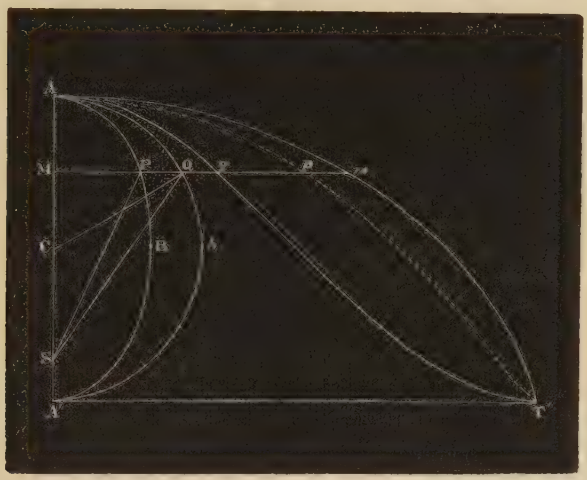
* See addendum to present paper.


A
T
m
n
l
k
a
M2
M1
C
S1
S2
S3
S4
a'
S5
S6
S7
S8
S9
A'
P2
P1
B9
B8
B7
B5
B3
B
C10
C9
C8
C7
C6
C5
C4
C3
C2
C1
q2
q1
P'2
P'1
b
b1
b2
b3
b4
b5
b6
b7
b8
b9
b10
C'
T'

Now if A m T′ be a cycloid having A A′ as its diameter, then

$$\text{Ordinate } M\,m = A\,Q + Q\,M$$

And if we take M q = A Q, we have q a point on A q T′, the curve of sines, otherwise called the companion to the cycloid.



The line $q\,m$ is then equal to Q M ; and if we take a point p on m Q such that

$$q\,p : q\,m :: C\,S : C\,A$$

we have

$$M\,p = A\,Q + \frac{e}{a}\,Q\,M.$$

accordingly we see (from α) that we may represent the time in traversing the arc A P by the ordinate M p to a curve A p T′, obtained by dividing all such lines as $q\,m$ (joining the cycloid and its companion, and paralled to A′ T′) so that $q\,p : q\,m$ as e to a.

Accordingly, if we construct such a diagram as is shown in the illustrative plate, in which A T′ is a semi-cycloidal arc and A b T its companion, while intermediate curves are drawn dividing all such lines as $b\,b_{10}$ into ten or any other convenient number of equal parts, the curves through the successive points b, b_1, b_2, &c., to b_{10}, give us the time-ordinates for bodies moving in ellipses having A and A′ as apses, and their centres of force respectively at C, S_1, S_2, S_3, &c. . . . S_9, and A′.

In the plate the semi-ellipses corresponding to these positions of the centre of force are drawn in, and it will be manifest that any ellipse intermediate to those shown can be pencilled at once, with sufficient accuracy. Ellipses within A B_9 A′ have their focus of force between S_9 and A′, and are exceptionally

eccentric.* It is easy to construct such an ellipse, however, in the manner indicated for the semi-ellipse A B_9 A′. For the radial lines and the parallels to A T through their extremities are supposed to be inked in; and (taking the case of ellipse A B_9 A′) we have only to draw the semicircle a B_9 a', and parallels to A A′ through the points where the radial lines intersect this semicircle, to obtain by the intersections of these parallels with the parallels to A T a sufficient number of points on the semi-ellipse.

The illustrative diagram has been specially constructed for the use of those who may have occasion to employ the method, and will be found sufficiently accurate for all ordinary purposes. Before proceeding, however, to show how the method is applied in special cases, I shall describe how such a diagram should be constructed :—

First the semicircle A B A′ must be drawn, and the lines A T, A′ T′ square to A A′. Then C A′ must be divided into ten equal parts (and when the figure is large, a plotting-scale for hundredths, &c. should be drawn). Next A′T and A T must be each taken equal to 3·1416 where C A′ is the unit. Join T T′. Now A T and A′ T′ represent, as time-ordinates, the half-period of any body moving in an ellipse having A A′ as major axis. Each must now be divided into the same number of equal parts, and it is convenient to have eighteen such parts. (So that in the illustrative case of our Earth, three divisions represent a month.) Next the semicircle A B A′ must be divided into eighteen equal parts. Through the points of division on the semicircle parallels to A T, A′ T′, are to be drawn,† and the points of division along A T and A′ T′ are to be joined by parallels to A A and T T′. Then the curve A b T′, the "companion to the cycloid," runs through the points of intersection of the first parallel to A T and the first to A A′, the second parallel to A T and the second to A A′, the third parallels to these lines, the fourth, and so on. We have now only to take $b\,b_{10}$ equal to C B; $q_1\,p_1$ equal to $M_1\,P_1$; $q_2\,p_2$ equal to $M_2\,P_2$; and so on, to obtain the required points on the cycloid A b_{10} T′; and the equidivision of all such lines as $b\,b_{10}$, $q_1\,p_1$, $q_2\,p_2$ (into ten parts in the illustrative diagram) gives us the required points on the intermediate curves.

Now let us take some instances of the application of the diagram.

I. Suppose we wish to divide a semi-ellipse of given eccentricity into any given number of parts traversed in equal times, and let the eccentricity be $\frac{1}{2}$, and 18 the given number of parts‡ :—

* It is manifest that when the centre of force is at A′ we have the case of a body projected directly from a centre of force, and the time-curve becomes the cycloid A b_{10} T′. Thus the above lines give a geometrical demonstration of the relation established analytically in my paper in the *Monthly Notices* for November 1871.

† Practically it is convenient to draw another semicircle on T T′, divide its circumference into eighteen parts, and join the corresponding points of division on the two semicircles.

‡ This selection is made solely to avoid the addition of lines and curves not necessary to the completeness of the diagram.

Then S_5 is the centre of force ; A B_5 A′ the semi-ellipse ; and A b_5 T′ the time-curve. Now the dots along A b_5 T′ give the intersection of the time-curve with the time-ordinates parallel to A A′, and therefore parallels to A T, though these dots (not drawn in the figure, to avoid confusion) indicate by their intersection with the semi-ellipse A B_5 A_1 the points of division required.

II. Suppose we wish to know how far the November meteors travel from perihelion in the course of one quarter of their period, that is, one half the time from perihelion to aphelion :—

The curve A B_9 A_1 is almost exactly of the same eccentricity as the orbit of the November meteors. To avoid additional lines and curves, let us take it as exactly right. Then A b_9 T′ is the time-curve. For the quarter period from perihelion (or aphelion), we take of course the middle vertical line, which intersects A b_9 T′ in c_9. This point by a coincidence is almost exactly in a parallel to A T, and this parallel meets the semi-ellipse A B_9 A′ in n, the required point on the orbit. In other words, the journey of the November meteors from A to n occupies the same time as their journey from n to A′, S_9 being the position of the Sun, and the Earth's distance from the Sun approximately equal to A′ S_9.

III. Suppose we require, in like manner, the quarter-period positions in different orbits, all having A A′ as major axis, but their centres of force variously placed along C A′. We get any number of points, n, l, k, precisely as n was obtained ; m, of course, is on the parallel through C_{10} ; and we obtain, in fine, the curve $m\ n\ l\ k$ B, which resembles, but is not, an elliptic quadrant.

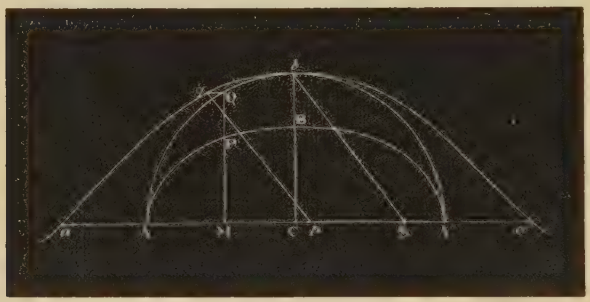
IV. Suppose we require to know in what time the half orbit from aphelion or perihelion is described in orbits of different eccentricity. The required information is manifestly indicated by the intersection of C C′ with the time-curves, in b, b_1, b_2, &c. Thus in the circle, A B is described in the time represented by C b ; in the semi-ellipse A B_3 A′, A B_3 is described in the time represented by C b_3, and B_3 A′ in the time represented by C′ b_3 ; and so on for the other semi-ellipses.

V. Suppose we require to determine approximately the "equation of the centre" for a body when at any given point of its orbit of known eccentricity. Take the case of *Mars*, whose eccentricity being nearly $\frac{1}{10}$, his path is fairly represented by the ellipse next within A B A′, and his time curve by A b_1 T′. Then the equation of the centre, when *Mars* is at his mean distance, is represented by $b\,b_1$; when *Mars* is at P_1 (not on the circle, but on the curve just within), the equation of his centre is represented by $q_1\ r_1$; and so on.

Many other uses and interpretations of the time-curves will suggest themselves readily to those who are likely to use the diagram.

After the above method had been briefly described, Prof.

Adams, who was in the chair, mentioned a method (devised by himself many years since) by which the same results can be obtained from a single curve,—the "companion to the cycloid" or "curve of sines." I have only Prof. Adams's *vivâ voce* sketch to guide me; but believe that I am right in saying that his method may be thus exhibited :—Let $a\,b\,a'$ be the y-positive half of one wave of the "curve of sines," b C its diameter, A b A′ semicircle with radius b C. Let A B A′ be a half-ellipse having focus at S.



Then time in any arc A P of this ellipse may be thus determined. Join b S, produce ordinate P M to Q, draw Q q parallel to $a\,a'$, and $q\,p$ parallel to b S; then does $a\,p$ represent the time in traversing A p, where $a\,a'$ represents the half period. And *vice versâ*, if we require the position of the moving body after any time from the apse, say aphelion, then take $a\,p$ to represent the time where $a\,a'$ is the half period, A C A′ the major axis, S the centre of force; join S b, draw $p\,q$ parallel to S b, q Q parallel to A A′, and Q P perpendicular to A A′ gives P the point required.

It will be manifest that in principle my method is identical with this, for in my figure the time is represented by M p, where M q is equal to the arc A Q, and $q\,p$ is equal to Q M reduced in proportion of C S to C A. Now $a\,p$ in the second figure is the projection of A q and $q\,p$; and the projection of A q is equal to the arc A Q, while the projection of $q\,p$ is equal to Q M reduced in the proportion of C S to A C.

But although Prof. Adams's construction, besides being earlier in publication, has the advantage of requiring but a single curve, yet mine subserves a distinct, and I think useful purpose. In fact, for the particular purpose I had in view, my construction alone avails. We see from the second figure that to give the relation between the times and positions in the case of the ellipse A p A′, we require a series of parallels to b C, $a\,a'$ and b S; and the parallels to b S only serve for this one case. Therefore we could not construct a reference figure for many cases, without having *many series of parallels* and a very confusing result. In my construction we have instead *many curves*, but a result which is not confusing, because each curve is distinct from the rest.

In fact, I may sum up the different qualities of the two constructions by saying that, whereas Prof. Adams's is the proper construction if a problem of the kind has to be dealt with *ab initio*, mine is, I conceive, the proper construction for a reference-figure. I repeat, however, that the principle of my construction cannot be regarded as new. Moreover, as a geometrical expression of the relation between the time and the position in elliptic motion, Prof. Adams's construction is manifestly superior.

On the Rejection of Discordant Observations.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In the *Monthly Notices* for April 8, 1868 (vol. xxviii. pp. 165–168), Mr. Stone suggested a criterion for the rejection of discordant observations, which it was the original object of this communication to examine. Before doing so, however, I wish to allude to a passage contained in a paper of mine on the subject of errors of observation, printed in vol. xxxix. of the *Memoirs* of the Society, in which the mode of treatment to which the theory itself leads is noticed, and to explain this method in greater detail than is there done. It will be seen that it supersedes the necessity for the *rejection* of anomalous observations.

The passage referred to occurs on p. 103, and consists of the quotation from De Morgan, and the remarks that precede it. As the extract from De Morgan is the foundation of what follows, and is very short, I here quote it again:—"Assuming the weights as nearly as they can be found, ascertain the most probable result, from which find the weights of the equations. If these agree with the assumed weights, the process is finished; if not, repeat the process with the new weights, and so on, until a result is obtained for which the assumed and deduced weights of the equations are sufficiently near to equality."

Believing this to be the true mode of completing the treatment of observations (and having been independently led to it), I proceed to develope its principles rather more in detail, and to answer some possible objections.

The problem we are concerned with is, Given a number of direct observations of the same quantity, determine the most probable value of that quantity. This is, of course, not the most general way of stating the question, as it is assumed that only one unknown is to be determined; but its discussion involves all the essential principles, and in a form free from unnecessary complication.

The usual reasoning is to suppose that all the observations

were made subject to the same law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, whence the arithmetic mean of the observation is the most probable value of the quantity observed. The value of h is then determined approximately from the observations, and thence the probable error.

Now, to the conclusiveness of this treatment there seems to be the fatal objection that h* is assumed to be the same for all the observations; viz. we act as if we had a reason for *knowing* that this must be the case; whereas, in point of fact, we have no *à priori* knowledge about the matter at all, and no *à posteriori* knowledge except such as we derive from the observations themselves. Our only ground for taking h to be the same for all the observations is the total absence of any reason for supposing, *à priori* (viz. before the observations are made), that any particular one will be better than any other; and the knowledge that if the number of observations were *infinite* in number, h would be the same for all.

But when the observations have been made, an examination of them cannot but afford information of some sort with regard to the probable values of the h's of the laws to which they were subject; and it is the information so given that the usual treatment ignores—an omission which gives rise to more or less arbitrary rules for the exclusion of certain of the observations, a clumsy expedient at best.

To state the matter more mathematically, if $V_1, V_2, \ldots V_n$ be the observations, the usual treatment gives for the most probable value of the quantity observed, a the arithmetic mean of $V_1, \ldots V_n$ and a certain value of h, say h_0, so that all the observations are supposed to have been subject to the law $\frac{h_0}{\sqrt{\pi}}e^{-h_0^2x^2}$; but, knowing that there is no necessity for the h's to be the same for all the observations, we see that the conclusion contradicts the premises, if we stop here; in other words, that a is only a first approximation; for a being a value very near the truth, then the errors of the individual observations are very nearly $a - V_1$, $a - V_2$, $\ldots$ $a - V_n$ (which call $v_1, v_2, \ldots v_n$), and it follows therefrom that V_i (v_i being supposed the numerically greatest of the series $v_1, v_2 \ldots v_n$) was most probably subject to a law in which h was less than the h of any other of the observations, and so on for the others. Thus the knowledge that a is in all probability very near the truth gives us the means of estimating and comparing the h's of the different observations; and we should proceed to weight them accordingly, and so to obtain a second approximation.

* I shall speak throughout of the h of an observation or a series of observations, and not of the corresponding probable error, as in contemplating an observation, the thing present to the mind is the law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ which more directly involves h than the probable error $\frac{\cdot 476936\ldots}{h}$.

This is, I apprehend, the kind of reasoning De Morgan had in his mind when he wrote the passage quoted above, having previously remarked that "we cannot refuse to allow that the establishment of a most probable result places us in the circumstances . . . . where we have to return upon our premises with new probabilities forced on us by the conclusion." (*Encyclopædia Metropolitana*, p. 456.)

The following investigation (in which, for clearness' sake, the process is developed quite from first principles) shows, I think, very plainly the way in which the weighting should be effected.

To fix the ideas, let an observation be regarded as accurate if it differ from the truth by an amount less than a fixed infinitesimal quantity k; then the chance of the accuracy of an observation subject to the law $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$ is $\frac{hk}{\sqrt{\pi}}$ and therefore varies as h, while the weight varies as h^2; so that the weight of an observation varies as the square of the probability of its accuracy.

The data are the observations $V_1, V_2 \ldots V_n$ and the knowledge that the law of facility which the errors follow must be of the *form* $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$, and the *quæsitum* is the most probable value of V.

In the first place, assume h to be the same for all the observations; then, if a is the true value of the quantity observed, the *à priori* chance that the n observations should be $V_1, V_2 \ldots V_n$ is proportional to

$$e^{-h^2\{(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2\}},$$

passing, therefore, from *à priori* to *à posteriori* probability, we have, after the observations are made, the chance that a is the true value proportional to

$$\frac{e^{-h^2\{(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2\}}}{\int_{-\infty}^{\infty} e^{-h^2\{(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2\}}\, da}$$

viz. to

$$e^{-h^2\{(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2\}}$$

so that the most probable value of a is obtained by making $(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2$ a minimum, viz. by taking a to be the arithmetic mean of $V_1, V_2, \ldots V_n$. (So far is, of course, Gauss's reasoning unaltered.)

But at the same time that this argument asserts that a is the most probable result, its probability being

$$A\, e^{-h^2\{(a-V_1)^2 \ldots + (a-V_n)^2\}},$$

it asserts that the probability of V_1 being the true result is

$$A\, e^{-h^2\{(V_1-V_2)^2 \ldots + (V_1-V_n)^2\}},$$

of V_2 being the true result is

$$A\, e^{-h^2\{(V_2-V_1)^2 \ldots + (V_2-V_n)^2\}}$$

&c., so that, calling these probabilities p_a, p_1, p_2, ... it follows that we are told that the weights of the observations V_1, V_2, ... are proportional to p_1^2, p_2^2, ...; and hence for our second approximation we so weight them, and obtain as result the value b, where b is such that

$$e^{-p_1^2(b-V_1)^2 - p_2^2(b-V_2)^2} \ldots$$

is a maximum. We can thus obtain, as before, new weights $p_1'^2$, $p_2'^2$, ...; and the process is repeated until the equations $p_1^{(n)} = p_1^{(n+1)}$, $p_2^{(n)} = p_2^{(n+1)}$ &c. ($p\,a$, $p\,b$ ... or the probability of accuracy of some one of the observations being always taken as unity, as only the ratios of $p_1^{(i)}$, $p_2^{(i)}$ ... are involved) are sufficiently nearly satisfied when the process is concluded, and then the best weights of the observations, and by consequence the best value of the quantity observed, have been obtained.

This seems the complete method of treatment of observations by the Theory of Errors, and it will be observed that the only assumption that has been made is that any error follows a law of the form $\frac{h}{\sqrt{\pi}}\, e^{-h^2 x^2}$. This, Laplace's proof (when modified, see Memoir previously cited, pp. 104, 105) seems to me to have established; but even if this were not admitted, the law, with the h determined separately for each observation, obviously can be made to represent the facts with a near approach to accuracy.

The calculation of p_1, p_2, ... the data of the second approximation presents no difficulty, for a being $\frac{1}{n}(V_1 + V_2 \ldots + V n)$,

$$(a-V_1)^2 + (a-V_2)^2 \ldots + (a-V_n)^2 = n\,a^2 - 2\,a\,.\,n\,a + \Sigma V^2;$$

so that the probability of a being the true result is $A\, e^{-h^2\{-n\,a^2 + \Sigma V^2\}}$, which call unity; so that the probability of any other value x is

$$A\, e^{-h^2\{n\,x^2 - n\,x\,a + \Sigma V^2\}} \div A\, e^{-h^2\{-n\,a^2 + \Sigma V^2\}}$$
$$= e^{-n\,h^2\,(a-x)^2},$$

as can be readily seen otherwise, though not in quite so elementary a manner.

This is, as I understand it, the method advocated by De Morgan; and all that I have done is to state it more definitely and in greater detail.

The sort of objections that I conceive might be raised against the completion of the method of least squares, as just indicated, would be (1) that in a series of observations made under apparently exactly similar circumstances (when obvious mistakes are expunged) we are bound to take h the same for all, and (2) that a kind of reasoning in a circle is involved in making the observations point out their own weight. To the first objection (which might also be enunciated in the form that h must not be altered unless there are grounds for believing in the existence of a disturbing cause affecting some of the observations) the answer is, that in our total ignorance of the *exact* way in which any one particular error does arise, we have not the smallest right to assume, except tentatively, that the h is the same; and this being so, the existence of an observation differing more than another from what we believe to represent the truth very nearly, *ipso facto* involves a presumption that the h for that observation was smaller. The reply to the second objection is, that the reasoning is of exactly the same character as that used in solving an equation or in reverting a series by repeated approximation.

It will be observed that the principles enunciated above are not in the slightest degree at variance with the method of least squares as usually used. I admit the whole of the ordinary treatment; only I go farther, and instead of saying, after the application of the rule, "This is the final result," I say, "No, this is not good enough for a final result, but it is good enough to weight the observations from;" and thence a better result is obtained, and so on.

Of course, if the first approximation is very nearly the best that the observations will give, the process of weighting will alter it very slightly; and it is to be understood that I make no proposition that observations should always be treated in this manner in practice — the work is sufficiently long as at present — but merely point out that the above is the proper mode of proceeding, if more accuracy be required.

I can imagine an objection of this kind: suppose a series of observations were made, and when the h had been determined as usual, they were found to group themselves almost exactly under the law $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$; would not this be strong evidence that the h was the same for all, and that any further weighting would be an unjustifiable tampering with the observations? To this the reply is that the observations never could exactly follow the law unless they were infinite in number, and that it is the deviations implied in the word "almost" which the weighting would help to

correct. If the number of observations were infinite, and exactly followed the law, the weighting would make no difference at all.

To consider the matter more fully, if an infinite number of observations had been made (assuming it, *argumenti gratia*, to be possible), and if they were found to group themselves exactly according to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, then it might be urged that this afforded a moral proof that this was the law, and that the arithmetic mean of the observations was the most probable value. In this limiting case, therefore, we have no occasion to weight the observations, even though some are very far from the truth; so that here the unweighted result is the best possible, and the method cannot rest on a principle universally sound. But the explanation is obvious, viz. that the arithmetic mean is the best result, because every positive error, of whatever amount, is exactly neutralised by a negative error of equal magnitude, so that no additional accuracy would be gained by weighting the observations, as no alteration of the result would follow therefrom. (This limiting case bears a rough resemblance to a series where all the terms become zero except the first.) The same takes place whenever corresponding to every error (or deviation from the arithmetic mean) there is an exactly equal error of opposite sign, as then the nth approximation coincides with the first; but in no case that actually arises in practice can it be true that the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ is accurately followed: we may compare the numbeer of rrors absolutely found between $0''{\cdot}0$ and $0''{\cdot}1$, between $0''{\cdot}1$ and $0''{\cdot}2$, &c. with the numbers given by the theory with the assumed value of h, and the agreement may be very close, indicating that the arithmetic mean is very near the truth, but this in no way shows that it *is* the most probable result; for in obtaining the arithmetic mean one comparatively large error may have neutralised several smaller ones of opposite sign—in fact, something of this sort always takes place, unless for every positive error there is always an equal negative one—and it does not follow, if an infinite number of additional observations were made under the same circumstances, and the curve of facility (or rather, in this case, the curve of frequency) of the whole, which we know to be of the form $y = \frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, laid down, that then the point where the middle ordinate cuts the axis of x would coincide with the point whose position was merely determined by the condition that the sum of the positive deviations from it should be equal to the sum of the negative deviations in the *finite* series of observations that were first made.

I have been thus particular in noticing this case because it seems to have been sometimes tacitly assumed that because, in a certain number of observations with an assumed h, we have a right to expect one or two large errors of a certain magnitude, therefore if these conditions have been realised in a series of ob-

servations, and if such large errors do appear, we are bound to retain them and give them the same weight as the others; the fallacy consisting in supposing that because such deviations might have been anticipated by the theory, therefore the theory is competent to treat them by itself, the same weight being given to them as to the others.

I may remark that to say the most probable law of facility for the observation V_i was $\frac{h_i}{\sqrt{\pi}} e^{-h_i^2 x^2}$ might seem, at first sight, a little startling, as, such being the case, we have but one observation subject to that law; but it must be remembered that we assume the law to be of the above *form*, and h_i^2 is simply the weight.

It is, perhaps, also as well to recall to mind here two things: (1) that Laplace's analysis (though he did not himself so apply it) only proves the law to be of the form $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$ if the actual error is a linear combination of an infinite number of smaller errors —this probably represents the facts sufficiently nearly—and (2) that, to adopt Leslie Ellis's terms, the rule of least squares, as usually used, is only the best method among methods, but need not give the best *result* among *results* in the same series of observations: extended as above, it should give the latter also.

From what has been said it is obvious that, if the above treatment were adopted, the rejection of an observation would never be necessary, except in the case where we saw it must be a mistake, and then the knowledge that would enable us to feel certain that this was so would remove the error from the category of errors of observation. An anomalous observation would obtain a very small weight, but never none at all: and this is as it should be. It is for this reason that in my Memoir I expressed my dissatisfaction with Pierce's criterion, viz. because I thought the principle of rejecting an observation *in toto* (except in the case of an obvious mistake) unsound, not because I thought the criterion itself specially objectionable. If we leave the strict course of mathematical procedure, we lose ourselves in the region of arbitrariness, as Gauss once said in reference to another matter; and if we will not approximate farther, but decide that equal weights shall be given to all the observations, if retained, we are compelled to find a criterion for rejecting some; for, of course, there frequently are certain observations which are prejudicial to the result, when retained having a weight equal to that of the best; so that the arithmetic mean would be nearer to the truth if they had never been made. Granting, then, that the middle course of weighting is not to be taken, and that all the observations are to have weight unity or zero, Pierce's criterion seems to me a very ingenious one; but still it is "arbitrary," viz. I presume others could be proposed having an equal claim to notice, as far as principle is concerned.

Mr. Stone's criterion, however, does not appear to be good, even among criteria, as it involves a principle which is, I think,

unsound. I may mention that I should have alluded to Mr. Stone's paper in my Memoir had I known of its existence; but it is only recently that Prof. Asaph Hall, of Washington, called my attention to it by the expression of an opinion unfavourable to the criterion suggested therein. It is necessary to briefly recapitulate the nature of Mr. Stone's argument, but after what has gone before detailed comment will be unnecessary.

After alluding to a number of error-producing causes, such as accidentally touching the micrometer-heads or the declination-screw after the bisections have been made, entering erroneously the thermometer or barometer readings (for the refractions), &c., due to the observer's want of care, Mr. Stone remarks that "we have here a series of causes of errors entirely distinct from those which lead to the ordinary graduated errors," and proceeds, "For a given class of observations, and for a given observer, there must exist a number n, which expresses the average number of observations which that person makes with one mistake: n may be defined as the modulus of carelessness. If, therefore, we find that value of p, which makes

$$\frac{2}{\sqrt{\pi}} \int_{\frac{p}{a}}^{\infty} e^{-y^2}\, dy^* = \frac{1}{n}$$

all larger values of p are, with greater probability, to be attributed to mistakes than to the ordinary run of graduated errors. Larger values of error than p must therefore be rejected."

In the first place it is to be remarked that the criterion cannot be intended as a practical one, as it is quite out of the question to make even a rough guess at the value of n; for (1) we do not know what sources of error are included, nor (2) within what limits the error is to be supposed to lie: it is to be "such a mistake as cannot be detected and corrected with perfect or almost perfect certainty;" and even were these two "unknowables" given, the determination of n would be sufficiently arbitrary. In fact, Mr. Stone himself acknowledges that it is "perhaps impossible to determine the absolute value of n," but points out, as a set-off, that the limits of rejection vary only slightly for a large change in the value of n. It is pretty clear, therefore, that it would be a good deal easier to choose a limit of retention at once than to guess a value of n and deduce one; so that I think I am quite right in regarding the criterion as a theoretical principle, and not as a merely practical rule: in fact, the author's own words are, "I have been led, on consideration of this subject, to a formula which embodies, in my opinion, the true grounds upon which my judgment rests when compelled to reject discordant observations."

The criterion seems to me most arbitrary, for why among all

* a is the reciprocal of the h of this paper.

the causes that can prejudice an observation should the carelessness (and only a particular kind of carelessness too) of the observer be the sole one to disqualify it from retention? Unless it could be shown that no cause could render an observation erroneous to a considerable* extent, except want of care on the observer's part, the principle fails; or, in other words, disturbing causes can arise in a variety of ways, and the criterion excludes from affecting the observations only such as have their origin in the carelessness of the observer. I am not here in any way straining the principle. Mr. Stone's own words are, "If this theory is correct, our rejection cannot be made except upon a direct admission of carelessness;" and this admission seems to me to pass sentence on the correctness of the criterion.

The principle comes to this, that the rule of least squares, as usually used, is competent to deal (*i. e.* without rejection) with errors over which the observer has no control, but is incapable of attending to errors that the observer could have avoided; and that a mathematical theory cannot draw so arbitrary a line is, I think, sufficiently evident *per se*, as it is hard to realise its discriminating between whether an error was due to the observer having written down 54 for 55, or to the fact of the observation having been made at a particularly unfavourable moment, when all the subsidiary errors happened to be of the same sign, and near their maxima. And this brings me to the only reasoning by which the principle could be defended, viz. by supposing that all the errors beyond the observer's control are continuous, and the errors due to carelessness, intermittent (viz. only operating once in n observations). Admitting this (though I have great doubts about its truth), the basis of the criterion seems to me equally fallacious, for the following reasons:—

An observation is to be rejected when there is reason to believe that we obtain a result nearer to the truth by its rejection than by its retention; and whenever it is considered that the result will be improved thereby, the observation is to be rejected, no matter to what cause its (presumed) large deviation from the truth may be due.

Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the very way contemplated by the theory and independently of the observer's care) and yet their retention may be quite as disastrous as if they had been mistakes made by the observer; so that the distinction between the two cannot be a proper ground for discriminating

* That is to say, considerable compared to the average magnitude of the continuous errors, but not greater than a continuous error can very well be. I purposely avoid using the term "graduated errors" to express those whose law of facility is continuous, as the adjective does not seem to me to convey to the mind the impression intended. Errors whose law of facility is continuous are here called *continuous* errors. An intermittent error, viz., one produced by a cause that operates only rarely, may have (and generally has) a continuous law of facility; but in this paper by "continuous" is generally to be understood continuous and non-intermittent.

whether an observation is to be accepted or rejected. If, on Mr. Stone's theory, the observer never makes mistakes (or, rather, only makes large ones), no observations are ever to be rejected; and this merely amounts to a statement, in other words, of the principle noticed above, viz. that the rule of least squares, as it stands, looks after all the ordinary continuous errors, however large, so that they need not be considered, but that we must treat in some other way errors arising from an intermittent cause. The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise; and in that case there is no distinction between the continuous and intermittent errors at all.

An example will illustrate the matter more clearly. Suppose that there were twenty coins, and that we tossed the whole twenty at one time, and, reckoning each head as + 1 and each tail as − 1, regarded the result as an error of observation (not as an observation itself) to which it bears a fair analogy. Let m tosses of the whole twenty be made, then the error of the arithmetic mean of the observations (whatever they may be) is $\frac{E}{m}$, E being the algebraic sum of the errors, and this quantity becomes zero when m is made infinite; but m being finite, say = 30, the question is whether we should not do better to reject, say, one particular observation, the error of which appears as, say, 18 in the register, than to retain it. The error in question may have arisen in two ways, either by the circumstance of nineteen heads having appeared, or by the observer having made a mistake in counting or registering the number of heads that did appear. If Mr. Stone thought it more likely to be due to the latter cause, he would reject the observation, otherwise he would retain it. But, however it got there, it is equally prejudicial to the truth, and if the attainment of the truth is our object, it must be rejected solely on account of its departure from the average, not on account of what we think may have been its origin. No matter how the 18 originated, it is one of the observed errors, and whether it owes its existence to the theory of chance (as a run of luck) or to a careless observer, it should be equally rejected from (or retained in) a finite series of observations. If more and more observations were made, the large positive errors would more and more be balanced by the negative ones (relatively to m); but then, when the number of observations is much greater than the observer's modulus of carelessness, the small mistakes merge into ordinary errors (for an error-producing cause that only acts once in n observations, and when it does act is subject to the law of facility $\varphi(x)$, is then merely regarded as subject to the law $\frac{1}{n}\varphi(x)$ where φ may be continuous or discontinuous).

I may also allude to another sentence in Mr. Stone's paper, viz. that in which he objects to a criterion proposed by Chauvenet for the rejection of a single observation, as "based on an erro-

neous principle ; for in 2 n such observations we ought reasonably to expect an error greater than or equal to x. In n observations therefore we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." Taking the words literally it is enough to reply that the question is not, how far we are surprised at such an error having been made, but whether it should be retained or not ; but the context shows that the real point involved is the distinction between avoidable and unavoidable errors, about which enough has been already said.

I must not be understood as expressing an opinion that the knowledge (if we had it) that an observer makes a small mistake in one out of every n observations ought to go for nothing in the consideration of what observations should be rejected and what retained ; if we have such knowledge it is an additional datum, and ought to be used somehow so as to make us more willing to reject discordant observations : but what I think is, that Mr. Stone has invoked a false principle, and drawn a fanciful line of distinction between errors, in his desire to make use of it. *Ceteris paribus*, I much prefer a criterion, such as Pierce's, where the disqualification depends simply on the magnitude of the error. More large errors always do practically occur than the continuous-error theory (with the same h) would lead us to anticipate ; so that if we determine to give every observation either full weight or no weight, some ought to be rejected ; but whether under these circumstances it is possible to propose a criterion, independent of caprice, I feel very doubtful.

I hope I have made my meaning clear, viz. (1) that it seems to me that the proper treatment of observations is by weighting them after having found an approximate most probable result ; (2) that it is only because we reject this method, and insist on giving the observations full weight or no weight, that the necessity for a criterion arises ; and (3) that granting we are to use a criterion, the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth. Of course when I say observations are not in practice weighted, I leave out of consideration any weight arbitrarily assigned by the observer, and refer only to such as the theory of errors itself suggests.

There is some little ambiguity about the meaning of the words "disturbing cause ;" the idea which seems to be generally attached to them is any source different to those that give rise to the ordinary continuous errors ; but the following is an example of the sort of way they are sometimes used : an observation departs a good deal from what we believe to be the truth, therefore it is bad, and to have rendered it so, some disturbing cause must have been at work ; under these circumstances therefore the observation cannot be recognised as forming one of the series to which the rest belongs, but must be rejected. A statement of this kind must be taken as a truism, and then it in effect contains the definition of a disturbing cause, making it mean

something or other which we presume does take place when the error is large. From this point of view therefore a run of luck is as much a disturbing cause as an observer's blunder, and so I regard it; but this is of course not the meaning that Mr. Stone and some others would assign.

I may, in conclusion, allude to what has been sometimes urged as an objection to the law $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$, viz., that an error, however large, is possible. But such ought to be the case in an infinite number of observations; for, even if we could limit the continuous errors, we can set no bounds to the blunders. In practice we reject the mistakes directly we recognise them as such, but in an infinite series of observations, where everything is left to the theory, it applies to them as well as to any other class of errors. In the case of a mural-circle observation one might suppose that no one could by any carelessness make an error of more than 180°, nor can any one in reading the instrument, but the observer might register the observation as 1105° instead of 105°, when a greater error is introduced. It is, of course, not true that a really infinite error is possible, as the probability is proportional to $e^{-\infty^2} = 0$. Another point deserving notice is the statement usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems at first sight not to be the case; for, suppose one were to estimate the area of a field containing, say two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero, or minus two (a case of this kind was pointed out to me by a pupil, in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark. The corresponding case in the theory of errors would be, if we had to measure a length so small that it was comparable with the errors of observation; a matter which would not in general be attempted. But if we were engaged with such a question, and a and b the abscissæ of the zero extremities of the length were observed, we should, of course, retain the values of $a - b$, whether positive or negative (viz. the mere fact of the length being negative, would be no ground *per se* for rejection).

Cambridge; 1873, *April* 2.

On the Eclipses mentioned in the Anglo-Saxon Chronicle.

By the Rev. S. J. Johnson.

Not having met with any work containing a description of the above, I have forwarded the following results to the Society

hoping they may prove of interest to some of the members. The computations I have made from the expeditious tables given in the *Encyclopædia Britannica*, but they may be expected to afford a very fair approximation to the truth. The edition of the *Saxon Chronicle* employed is that by Thorpe, published under the direction of the Master of the Rolls in 1861.

A.D. 538. "In this year the Sun was eclipsed fourteen days before the Calends of March from early morning till nine." I believe this is notable as the first eclipse of which we have any record as occurring in this country. I make the greatest obscuration to have amounted to two-thirds of the Sun's diameter at London about $7^h\ 43^m$ A.M. on Feb. 15. In Tycho Brahe's *Historia Celestis** it is referred to in these words, "Eclipsis solis annotata invenitur apud scriptores Anglos, quæ acciderit die XV Calend. Martii sive XV Feb. anno quinto Henrici regis West-Saxonum in Anglicâ, hora diei primâ usque fere ad tertiam sive statim post ortum solis."

540. "In this year the Sun was eclipsed on the 12th of the Calends of July, and the stars appeared full nigh half an hour after nine." The middle of this eclipse comes out at London about $7^h\ 37^m$ A.M., and the magnitude very nearly the same as in 538. It is mentioned in T. Brahe's *Historia* and in Struyk's list of eclipses given in Ferguson's *Astronomy*. Both give a similar magnitude to that I have obtained. The semi-diameter of the Sun was nearly as small as it can be, that of the Moon nearly as large as possible. The question arises, How are we to explain the notice of the stars showing themselves when totality would take place far south of England? I can only say the account must be taken from the narrative of those who saw it where the magnitude was greater than in this country.

664. "In this year the Sun was eclipsed on the Vth of the Nones of May; and Earcenbryht, king of the Kentish people, died, and Ecgbryht his son succeeded to the kingdom." On May 1st I find a very large partial eclipse of the Sun, the maximum at London soon after 5^h P.M. with a thin crescent uncovered at the south of the Sun's disk. It would seem to have been total in this country.

733. "In this year Ethelbald captured Somerton; and the Sun was eclipsed, and all the Sun's disk was like a black shield; and Acca was driven from his bishopric." The eclipse must have been upon August 14th, when I find a very large one took place, which according to these tables was annular in England, the greatest phase at London about a $\frac{1}{4}$ past 8^h in the morning, when I find for the Sun's semi-diameter, $15'\ 54''$, the Moon's, $15'\ 22''$. Tycho Brahe's *Historia Celestis* speaks of it as "circiter horam diei tertiam pene totus orbis solis nigerrimo et horrendo seuto videretur obtectus." In Humboldt's *Cosmos*, vol. iii. part ii. seventeen instances are given in a note, of sudden diminutions of

* There is a fine copy of this work in the Library of the Society.

the light of day, Humboldt treats them as meteorological phænomena, and doubtless some may be disposed of in that way, as, for instance, that in 45 B.C. about the death of Cæsar. But I think in his notice under 733, the above eclipse is alluded to, "A year after the Arabs had been driven back beyond the Pyrenees as the result of the battle of Tours, the Sun was darkened on the 19th of August in a terrifying manner."—Schnurrer, *Chron.*, Th. 1, S. 164. This must be the first annular eclipse in England, of which we have any record.

795. "In this year the Moon was eclipsed between cock-crowing and dawn on the Vth of the Kal. of April; and Eardwulf succeeded to the kingdom of the Northumbrians on the IInd of the Ides of May." The eclipse mentioned must be that of March 28, 796. In the former March I find no eclipse, but on the morning of the latter date, one began about 4^h, and the Moon would set during the total phase.

802. "In this year the Moon was eclipsed at dawn on the XIIIth of the Kal. of January (Dec. 20)." Some mistake about the date. The full Moon in Dec. 801 was on the 23rd, and was not eclipsed, nor were the full Moons in Dec. 800 or 802. I find an eclipse on the XIIIth of the Calends of June, not January, or in the night of May 20–21, 802, which I fancy must be that alluded to. It commenced about 2.20 A.M., and was nearly total a few minutes before sunrise.

809. "In this year the Sun was eclipsed in the beginning of the fifth hour of the day, on the XVIIth of the Calends of August, on the second day of the week, the 29th of the Moon." I find an eclipse on July 16th, not however very remarkable, greatest phase by these tables about 9.22 A.M., magnitude 7-tenths on the Sun's upper limb. By the fifth hour of the day we must no doubt understand the fifth hour from sunrise. So the account in the Chronicle is very exact.

827. "In this year the Moon was eclipsed on midwinter's mass-night, and the same year King Ecgbryht subdued the kingdom of the Mercians, and all that was south of the Humber." The date given is wrong again. The eclipse on the morning of Christmas Day 828, must be here meant. It commenced about a quarter past midnight, and after passing through a total phase, ended about a quarter to four in the morning. This must have taken place about the time when Egbert triumphed over opponents and united the several Anglo-Saxon nations into one powerful monarchy.

879. "The Sun was eclipsed one hour of the day." No month is given. By examining the new Moons I found the Sun eclipsed only so as to be invisible to England this year. But in 878 there was a great eclipse on October 29, which I have little doubt is the one here referred to. T. Brahe's *Historia Celestis* gives the following notice of it, "Ait autem auctor vitæ Ludovici solem post horam nonam ita obscuratum esse, ut stellæ in cœlo apparerent, et omnes sibi noctem imminere putarent." The tables

in the *Encyclopædia* gave totality at London about $1^h\ 14^m$; and Mr. Hind has informed me by a more recent and rigorous computation that totality came on at $1^h\ 6^m\ 20^s$, and lasted nearly two minutes. A note in Thorpe's translation of the Chronicle says, "The eclipse happened on March 14, 880," but as that eclipse was at sunset, and nowhere total, it can hardly be here signified. I have examined each year from 878 to 1715, and have not succeeded in finding another eclipse that appears to have been total at London.

1110. "On the fifth night in the month of May, the Moon appeared in the evening, brightly shining, and afterwards by little and little its light waned, so that as soon as it was night it was so completely quenched, that neither light nor orb, nor anything of it was seen. And so it continued very near until day, and then appeared full and brightly shining. It was on this same day a fortnight old. All the night the air was very clear, and the stars over all the heaven brightly shining. And the tree-fruits on that night were sorely nipt." I find the Moon, by these tables, entered the Earth's umbra about 9^h, and emerged from it about $12^h\ 30^m$. The expression "neither light nor orb, nor anything of it was seen," is curious, and seems to me to indicate this as a case of the disappearance of the Moon during totality, perhaps the first recorded instance of that peculiarity. Humboldt in *Cosmos* (Art. "Solar Domain") says, "In total lunar eclipses it happens in some exceedingly rare cases that the Moon disappears wholly; it did so according to Kepler's earliest observation on the 9th of December, 1601." Humboldt's and other popular works give four other instances of this, 1620, 1642, 1761, 1816. On calculating the eclipse of 1601, I find it was not total, about nine-tenths of the Moon's surface dipping into the Earth's shadow. In a curious old work in my possession by Cyprianus Leovitius, the Bohemian astronomer (entitled *Eclipsium omnium ab anno* 1554 *usque ad annum* 1606 *accurata descriptio et pictura*), this is drawn as an eclipse of 11 digits. I see there is a long description of it in the *Paralipomena* to T. Brahe's *Hist. Celest.*, but no mention of the disappearance of the Moon. In 1620, however, two total lunar eclipses are mentioned in that work, one on June 14, the other on December 9, in both of which the Moon is stated to have shown with peculiar obscurity during the total phase. It seems to me, therefore, that astronomical writers in future must regard Dec. 9, 1601, as a mistake for Dec. 9, 1620. Of the eclipse of June 14, 1620, it is said, "The Moon was seen with great difficulty, it shone moreover like the thinnest nebula, far fainter than the Milky Way, without any copperytinge" (*rubedine*). About the middle of the second hour nothing could be seen of the Moon with the naked eye, and through the telescope (*tubum*) something was detected so uncertainly that no one could tell whether the Moon was not something else. Of the Eclipse of Dec. 9, there is this entry, "Deinceps luna omnino disparuit, ut nihil de eâ videretur, clarè aliis circumcirca stellis

lucentibus, et sic exul, amissaque luna perstitit uno quadrante paulo plus." *

I have omitted seven other lunar eclipses mentioned in the *Chronicle* as the dates given agree with the calculation, and they present no special feature of interest. The total solar eclipses of 1133 and 1140 have been so thoroughly described by Mr. Hind that there is no need to refer to them. In each case by projecting for London with elements from these tables, I obtain a small crescent at the south of the Sun's disk.

Upton-Helions Rectory, Crediton, Devon,
February 22.

Note on the Distribution of Resolvable and Irresolvable Nebulæ. By Sidney Waters, Esq.

(*From a Letter to Mr. R. A. Proctor.*)

Whilst studying the nebulæ and their distribution it occurred to me that if the resolvable and irresolvable nebulæ were charted down with some prominent distinctive characteristic, light would be thrown on the question, 'Is resolvability any test for distance?'

Lately I have begun to test this, and have met with such remarkable results that I am induced to trouble you with a few remarks. First, I would state that I copied the meridians and parallels laid down on Map 2 of your large Atlas, only altering the hours of R.A. from 22, 23, 24, &c., to 10, 11, 12, &c., it appearing that this map then included the most interesting nebular regions; and, further, having marked in the meridians and parallels to every 4 minutes in R.A., and every degree in N.P.D., I plotted in the nebulæ from Herschel's catalogue, marking in the resolvable nebulæ in red ink, the irresolvable nebulæ in black (by resolvable I include all objects marked *r*, *rr*, *rrr*, *e r*, Cl). This done, a glance shows, that there is a most remarkable connexion between the resolvables and the irresolvables, that where the irresolvables cluster there also do the resolvables.

The following statement seems to confirm this view. The whole map contains about 1284 nebulæ, of which 157 are resolvable; this is about the proportion of 10 resolvables to 80 irresolvables.

A space bounded by R.A., 12^h and 13^h, and N.P.D. 70° and 90°, contains 266 nebulæ, of which 49 are resolvable; this is in about the proportion of 10 to 54, but this space occupies only about $\frac{1}{15}$th of the map, so that if the nebulæ were as richly spread

* In the Eclipse of July 12, 1870, I noticed the Moon's disk much duller and the coppery tinge much fainter than is usual during totality. From 9.55 to 10.15 I was unable to detect some portions of the eastern limb, either with the naked eye or telescope.

in other parts, there would be in the map 3990 instead of 1284 observed; and if the resolvables were as richly spread we should have 735 instead of 157 observed. It will be seen that while this small space is extraordinarily rich in irresolvable nebulæ, its richness in resolvable nebulæ not only increases in proportion, but in a far greater ratio. (I may note that if the richness of the whole heavens were compared with the richness of this small space, the contrast would be even more striking.)

The coincidence of this great cluster of irresolvable nebulæ with the relatively greater cluster of resolvable nebulæ of course cannot be ascribed to chance.

I have not found that this close association between resolvable and irresolvable nebulæ has been noticed before, although you advocate the conclusion to which it points on more general grounds in your paper in 1869 (Supplementary Number of the *Monthly Notices*).

Oakhurst Lodge, Tufnell Park,
Holloway, N., *March* 8, 1873.

The Meteor Shower of 1872, *November 27th.*

By Professor George Forbes.

Seeing that this shower probably had an intimate connexion with Biela's Comet, I laid before me three objects:—the determination of the radiant-point, the indication of the tracks of more brilliant meteors, and data for determinating their velocity.

1. *Determination of the radiant-point.* Being desirous of determining not merely the point of radiation, but also whether the radiation were good, I proceeded in the following manner. I marked in my note-book, as accurately as possible, the positions of the chief stars in *Cassiopeia*, *Perseus*, and *Andromeda*. Upon this were drawn the tracks of *all* the meteors coming within that range (except when several appeared at the same instant). After observing in this manner for three hours, those alone were marked that appeared close to the point of radiation.

The tracks were afterwards carefully copied on an accurate star-map, and the lines indicating the paths were produced backwards through the point of radiation. In this manner the radiant-point can be determined by the relative blackness caused by so many black lines intersecting. Hence nothing is left to the eye's judgment. 112 tracks were thus treated. The chart was photographed, and a copy is forwarded herewith. The parallels of North Declination 45° and 50° are indicated, as also the lines of R.A. from 1^h to 2^h. The densest part of the intersection of black lines is at R.A. $= 1^h\ 35^m$, N. Decl. 46°. This I conclude is the radiant-point. A perfectly stationary meteor appeared at R.A. $1^h\ 40^m$, N. Decl. 46°.

The radiation is on the whole good. That is to say, there are few meteors whose tracks diverge far from this point.

2. *Tracks of brilliant meteors.* Most of the meteors were equal to stars of the third magnitude; some of the second, a few of the first or brighter. The time of appearance of these last only was recorded.

No.	Time	Beginning. R.A.	N. Decl.	End R.A.	N. Decl.	Duration.	Remarks.
	h m	h m	°	h m	°	s	
1	7 22	0 40	52	22 20	62	3	
2	7 34	1 10	35	0 40	27	1½	
3	7 39	1 0	35	0 20	27		
4	8 5	2 0	40	2 30	30		Longer than usual; very brilliant.
5	8 6	2 40	55	3 40	58		= Venus; train lasted 5m; drifted to N.E. (wind was from S.W.)
6	8 25½	1 10	57	0 50	68		= 1st magnitude.
7	8 51	In the south east.					Burst.
8	9 8	2 40	55	3 40	58		Very brilliant; train red and green.
9	9 33½	3 0	52	4 0	52	2½	

The watch used was 30s slow.

3. *Data for determining the velocity.* Besides the data in the last paragraph, which will be useful when the average height has been determined, I made the following determinations of meteors which seemed to have a normal velocity.

Distance from Radiant Point. At Beginning.	At End.	Duration.
°	°	s
4	9	1½
4	10	1½
9	18	2
7½	14	2½

The night was very clear.

Anderson's University, Glasgow.

Observations of Meteors and Meteoric Showers of Nov. 1872. By Captain Chimmo, of H.M.S. "Nassau."

(*Communicated by Capt. Toynbee.*)

The following extracts are from the log of H.M.S. "Nassau," Capt. W. Chimmo, I send them to you as I know some of the Fellows are working at the subject of meteors.

"13th July, 1872, 2 A.M. Observed a brilliant meteoric body fall vertically, and splash into the sea on the port beam. Position at noon, 2°15′ N. 124°30′ E.

In Bombay.

24th Nov. 1872. Midnight, observed a meteor pass from south to west.
25th, 10 P.M. Observed a meteor pass from N. to N.E.
26th, 10 P.M. Observed a meteor pass from N.N.E. to east.
27th, 8 P.M. Observed a most unusual meteoric shower lasting 8 hours, counted 300 in five minutes.
29th, 6 A.M. Observed several small meteors.
30th, 6 P.M. Observed several small meteors."

Meteorological Office,
116 Victoria Street, London, S.W.
January 17, 1873.

Meridian Marks for Transit Instruments.

By Edward Crossley, Esq.

In considering over the best means of using a meridian mark, near at hand, for a small transit instrument, where it is inconvenient to mount either collimating telescopes or *fixed* plain lenses, ground to the focus of the distant mark, on stone piers, or on the walls of the obsrvatory itself (which may be only of woodwork), it has occurred to me that, supposing the mark be near at hand, say 50 feet or more, a plain lens ground to the focus of this mark may be slid on in front of the object-glass of the transit instrument by means of a firm adapter, and in place of the dew-cap usually supplied (the weight of the two being equal) the balance will remain undisturbed.

The distant mark will now be distinctly visible in the field of the telescope, and should the plain lens not be exactly central with the mark, a reversal of the transit instrument will show the difference, provided the telescope has already been duly collimated over the mercury trough.

It will be evident that the above method will give great accuracy to a portable instrument, with or without observatory; it being only necessary to place the mark at the right distance in the meridian from the object-glass. Where the telescope is mounted at one end of the horizontal axis, there should be two meridian marks at a distance apart equal to twice the excentricity of the telescope.

With regards to mercury troughs, perhaps some of your readers may not be aware of the advantage of a shallow copper trough, say $\frac{1}{16}$-inch in depth just covered with mercury. It is wonderfully still on a firm support, and yet perfectly sensitive. It is convenient to place the copper trough inside a larger wooden trough to hold the excess of mercury.

Bermerside Observatory,
Halifax, March 5, 1873.

Re-discovery of Tempel's Comet of Short Period. Letter from M. Stephan, Director of the Observatory at Marseilles, to Mr. Hind, dated Marseilles, April 5.

"I am very much obliged by your having sent me the ephemeris of the comet of Tempel (1867 II). The state of the sky has not allowed of my utilising it, until the night before last, when I immediately found the comet, not far from the place assigned by Dr. Seeliger. My observation is as follows:—

At $15^h\ 26^m\ 24^s$ M.T. at the New Observatory.
App. R.A. $16^h\ 26^m\ 21^s{\cdot}57$; App. N.P.D. $100°\ 39'\ 12''{\cdot}5$.

Mean Position of the Comparison-Star for 1873·0.
W. H. XVI. 579 $16^h\ 31^m\ 37^s{\cdot}20$ $100°\ 35'\ 24''{\cdot}8$ Weisse's Cat.

The comet is excessively faint. I saw it again for an instant last night, but the sky was very vaporous, and it was not possible to obtain another exact position."

The corrections to Dr. Seeliger's ephemeris indicated by the Marseilles observation are—

In R.A.	$-3^m\ 28^s$
In N.P.D.	$+\ 5'{\cdot}3$

On a large Automatic Spectroscope. By John Browning, Esq.

I have just completed for Mr. Gassiot a large spectroscope for presentation to the Oxford University; and the instrument having been pronounced by the highest authority we have in England the most perfect and most powerful instrument of the kind yet made, I have thought a very brief description of it might not be without interest to many Fellows of the Society.

The great dispersive power of the instrument—which I have now the honour of exhibiting before you—is obtained by a battery of six compound prisms, 3 inch high, and 2 inch wide, the light, after passing through the upper half of these prisms, is reflected back through the lower half; and the size of the instrument will be best imagined when the fact is stated that the light, in its passage through these prisms, has to pass through more than 4 feet of glass before it reaches the eye of the observer. The telescopes are of 18-inch focal length, and the object-glasses $13\frac{1}{4}$-inch in diameter. The prisms are provided with the automatic arrangement for keeping them at the minimum angle of deviation for any ray under examination, which I have already had the honour of describing in the Proceedings of the Society. It is intended that all the measuring of the spectra should be done by means of a micrometer eye-piece placed in the telescope; but for the purpose of readily finding any line in the spectrum, the prisms are provided with a vernier, which moves round the circular arc. The divisions

are on an alloy of palladium with silver. There is a contrivance for setting the train of prisms in motion, the milled head which moves the prisms being close to the eye-piece of the telescope, and thus completely under the command of the observer. Though every part of the instrument has been made as light as it well could be consistently with strength, the instrument weighs rather more than 140 pounds.

On a Spectrometer. By John Browning, Esq.

Several months since Colonel Campbell suggested to me a simple method of mapping out the spectrum by attaching a quick-threaded screw to the slow-motion screw of a fine micrometer. I regret that so long a time should have elapsed before I saw the full value of this ingenious plan. I have now the pleasure of exhibiting a contrivance made on this plan; it will be seen that the contrivance can be attached to an ordinary micrometer; a quick thread carries a small frame between brackets, similar to the carrier of the dividing point of an ordinary dividing-engine. The method of using this contrivance is as follows: A piece of smoked glass is inserted in a metal frame under the pointer; on bringing the micrometer-wires to coincide with any line, a corresponding line can be made by a stroke of the pointer on the smoked glass, and this proceeding can be carried on with as many lines as may be in the field of view. When used on an automatic spectroscope, the prisms should be moved, and the mapping continued until as many lines are registered as the glass will contain. To avoid loss of time in the quick-threaded screen, the carriage is kept to one bearing by the pressure of a spring. After taking the glass out of the frame, it may be varnished in a manner similar to that employed for varnishing photographic negatives.

Phenomenon observed at Sea. By Commander H. P. Knevitt.

(*Communicated by Rear-Admiral Richards, Hydrographer.*)

A phenomenon having been seen from this ship when on the passage from Manzanilla to this place, I think it advisable to make it known at once, instead of waiting the transmission of my remark-book at the end of the year.

On the 16th May, 1872, at $2^h\ 45^m$ A.M. (the weather having been squally since midnight) a phenomenon was seen in the heavens at an altitude of about 50° and bearing east of compass; the ship at the time being in lat. 14° 55′ N. and Long. 99° 58′ W.

I did not see it myself, but the following is a description given of it by Lieutenant Cecil G. Horne, who was officer of the watch:—

"Attention was first drawn by a very bright flash, resembling a small flash of vivid lightning, but being much more solid, and

lasting 4 to 5 seconds; the passage of the luminous body was towards the horizon for a short distance (say 3° or 4°), in a zig-zag course; it then appeared to burst and throw off a tail such as a comet has; the tail forming a ring and spreading itself round the body, till the whole had very much the appearance of a large Catherine wheel; it then gradually faded out of sight, having been visible from first to last, about 10 to 15 *minutes*."

H.M.S. "Fawn," Panama,
11th June, 1872.

Observation of Transit of Jupiter's fourth Satellite.
By G. W. Roberts, Esq.

March 26th, 1873. Observed *Jupiter* about 8 P.M. and found the fourth satellite on the disk. I thought at first it must be a shadow, but on referring to *Nautical Almanac* found that it was the fourth satellite itself. A friend was observing with me, and we both agreed that it was a very intense black, and also was not quite round. We each made independent drawings, which agreed perfectly, and consider that the observation was a perfectly reliable one. The shadow appeared not quite round, but two segments were wanting (see engraving). We could not imagine that such an intense black object could be visible when off the disk, and waited with some impatience to see the emersion, but were disappointed by fog which came on just at the critical time. The satellite when on the disk seemed larger than when off the disk and shining brightly at ordinary times.

Telescope 8-inches aperture, achromatic powers 135, 249, 368, and 533. Definition very good.

Should be glad to hear if any one saw the emersion, and could see the satellite with any small aperture.

April 7th. Observed *Jupiter* with 8 inches aperture, and the fourth satellite was very faint indeed.

Recent Measures of ξ Ursæ Majoris, ζ Cancri, and μ_2 Boötis (Σ 1938). By J. M. Wilson, Esq.

The following measures of these binaries have been obtained during the last few days of March and the first few days of April. It may be interesting to compare them with the ephemerides published in previous numbers of the *Monthly Notices*.

	Position.	Distance.	Epoch.
	°	″	
ξ Ursæ	3·93	0·90	1873·22
ζ Cancri A.B.	150·9	0·5 to 0·6	73·22
μ_2 Boötis	151	0·4 to 0·5	73·25

The two former positions are the means of 25 and 23 observations respectively made on five nights by Mr. Seabroke and myself; the third is the mean of four readings by the same observers.

Temple Observatory, Rugby.